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THE RATIO OF THE CAPTURE CROSS-SECTIONS OF LITHIUM AND BORON FOR THERMAL NEUTRONS[1]

BY F. W. FENNING,[2] G. A. R. GRAHAM,[3] AND H. SELIGMAN[2]

Abstract

A small boron trifluoride chamber is used to obtain the spatial distribution and density integrals of thermal neutrons produced by a source in aqueous solutions of compounds of lithium and boron. The effects of the finite size of the boron chamber are minimized by adjusting the concentrations of the solutions so that the mean life of thermal neutrons is the same in both. The scattering mean free paths are also almost identical. The final result is

$$\sigma_{Li}/\sigma_B = 0.0948 \pm 0.0013.$$

Taking $\sigma_B = 705 \times 10^{-24}$ cm.^{2} per atom, $\sigma_{Li} = (66.8 \pm 1.0) \times 10^{-24}$ cm.^{2} per atom.

Introduction

Early measurements of the capture cross-section of lithium for thermal neutrons are in considerable disagreement, largely owing to uncertainties in the mean energies of the neutron spectra concerned. Usually the capture cross-sections for boron and lithium have been determined by the same authors with the same geometry and spectrum, so that even if the values for any one of these substances are in disagreement, the ratio σ_{Li}/σ_B should be independent of the spectrum if we can assume a $1/v$ variation of cross-section with neutron velocity for both substances. However, even these ratios do not agree, so that when certain experiments requiring knowledge of the cross-section of lithium were planned in this laboratory, it was also decided to determine the ratio σ_{Li}/σ_B by the method used earlier by Frisch, Halban, and Koch (2), and by ourselves in determining σ_B/σ_H (1).

As in the work mentioned above (1), a small boron chamber is used to obtain integrals of the thermal neutron density produced by a source in aqueous solutions of compounds of the elements concerned. The chief source of error in this method comes from the effect of the finite size of the boron chamber, which effect may be different in two solutions of different capturing and scattering properties. In the present experiment these effects are brought to a minimum by adjusting the concentrations of the solutions used (one

[1] *Manuscript received August 29, 1946.*

Contribution from the Nuclear Physics Branch of the Montreal Laboratory, Atomic Energy Division of the National Research Council of Canada. Montreal report, MP-71, dated June 29, 1944. Issued as N.R.C. No. 1497.

[2] *Member of United Kingdom staff.*

[3] *Physicist.*

containing lithium, the other containing boron) so that the mean life of thermal neutrons is the same in both. In the two solutions used the scattering mean free paths were also almost identical.

Apparatus

The boron chamber was prepared by Mr. N. Veall and has been described (5), its dimensions being 0.6 cm. in diameter and 2 cm. in length. It was mounted on a brass tube through which the lead from the collecting electrode passed to the head amplifier. The high tension lead passed through a glass tube a few centimetres from the brass tube.

Two identical tanks, 80 by 40 by 30 cm., contained the two solutions. A frame which was not attached to any tank and which carried the source and the chamber was used for the measurements in both solutions. Source and chamber could move on rails fixed to the frame. The distance between source and chamber was read off a mirror scale attached to the frame behind indicators fixed to the source and chamber supports.

The source was contained in a lead and brass box (1 cm. of lead, 0.1 cm. of brass). The lead served to reduce the 'noise' due to γ-rays in the chamber. The usual linear amplifier and scaling circuits were used for counting the impulses from the boron chamber.

Experimental Procedure

On the basis of existing data, solutions of boric acid and lithium acetate were so prepared as to reduce the mean life of thermal neutrons in each to about one-fifth of that in water. In these solutions boron chamber counts

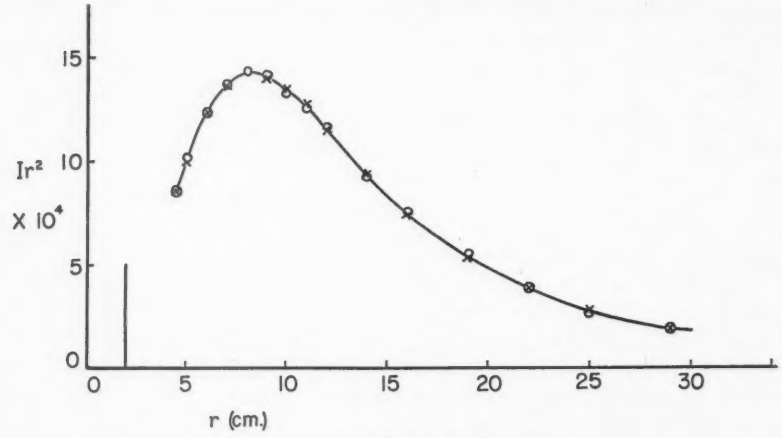

FIG. 1. *Spatial distribution of thermal neutrons around a Ra-α-Be source (0.6 gm. Ra). Solution I: boric acid in water (C_B = 0.197 gm-atoms per litre). Measurements shown by circles. Solution II: lithium acetate in water (C_{Li} = 2.095 gm-atoms per litre). Measurements shown by crosses.*

were taken at several distances from the source. From the ratio of intensities in the two solutions for given separations of source and chamber it was possible to see by how much the concentration of one of the solutions would have to be changed in order to bring their capture probabilities to the same value. After a few preliminary adjustments of this type, complete density distribution curves were obtained by plotting Ir^2 against r for each solution. I represents the density of thermal neutrons at a distance r from the source, measured in counts per minute from the boron chamber. Sufficient counts were taken to give better than 1% statistical accuracy to the more important points, while the weaker intensities far from the source were progressively less accurate. To safeguard against drift in the counting apparatus, measurements were made in the two solutions alternately, but no significant change was apparent.

The final measurements were carried out in the course of one day. Immediately after these measurements were finished, samples of the two solutions were taken for chemical analysis. We wish to thank Mr. A. C. English and Mr. J. Hébert for carrying out these analyses of the lithium and boron content.

Results

It can be seen from the curve of Fig. 1 that to the accuracy of the measurements the two solutions had the *same* mean life for thermal neutrons. Then the mean life τ is inversely proportional to $\sum_i C_i \sigma_i$, where C_i represents the concentration in gm-atoms per litre of a capturing nucleus of cross-section σ_i.

$\therefore C_B\sigma_B + C_H\sigma_H = C_{Li}\sigma_{Li} + C'_H\sigma_H$, if we neglect the small amount of absorption in oxygen and carbon present.

From the analysis $C_B = 0.197$ $\quad C_{Li} = 2.095$

$C_H = 110.7$ $\quad C'_H = 107.0$.

$$\therefore \frac{\sigma_{Li}}{\sigma_B} = \frac{C_B}{C_{Li}} + \frac{(C_H - C'_H)\,\sigma_H}{C_{Li}\,\sigma_B} = 0.09403 + 0.00078 = 0.0948,$$

using the known value of σ_H and taking $\sigma_B(kT) = 705 \times 10^{-24}$ cm.^{2} per atom (private communication from the Chicago group early in 1944) to obtain the hydrogen correction term.

Hence $\sigma_{Li}\,(kT) = 66.8 \times 10^{-24}$ cm.^{2} per atom.

Recently it has been shown that both lithium and boron have capture cross-sections that are inversely proportional to the velocity of the neutrons (3, 4). Therefore it follows that the ratio of their capture cross-sections is independent of the neutron spectrum.

Accuracy of Measurement

Previous measurements of this type have frequently shown that a particular density distribution curve can be reproduced to an accuracy of about $\frac{1}{2}$% in the area underneath the curve. Hence it is reasonable to say that the mean life in the two solutions was the same to about 0.7%. The chemical analyses were carried out by two independent observers by slightly different methods,

each observer giving a reproducibility figure of about $\frac{1}{2}\%$. Their results actually differed by about 1% in absolute value, but only $\frac{1}{4}\%$ in the ratio C_{Li}/C_B. Hence from errors in the analysis we may expect an uncertainty of about 0.7% in the ratio σ_{Li}/σ_B. Consequently we get

$$\sigma_{Li}/\sigma_B = 0.0948 \pm 0.0013,$$

or taking $\sigma_B = 705 \times 10^{-24}$ cm.^{2} per atom,

$$\sigma_{Li} = (66.8 \pm 1.0) \times 10^{-24} \text{ cm.}^2 \text{ per atom.}$$

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EVIDENCE FOR AN (n, α) REACTION INDUCED IN O^{17} BY THERMAL NEUTRONS[1]

By A. N. May[2] and E. P. Hincks[3]

Abstract

It can be shown from the masses involved that the reaction O^{17} (n, α) C^{14} should be exothermic by 1.72 Mev., and should therefore occur with thermal neutrons. As an attempt to detect the reaction an investigation was made of the pulse spectrum produced when an ionization chamber containing oxygen was placed in a flux of thermal neutrons. The pulses were amplified and then counted in a series of energy bands by means of a kicksorter. Two oxygen samples were compared, one being enriched (three times normal), and the other impoverished (0.5 times normal), in O^{17}. Differences between runs with these two gases showed a considerable number of pulses between 1 and 2 Mev., with a maximum at 1.4 Mev. Reasons are given for assigning these pulses to the above reaction, including considerations of the possibility that other exothermic neutron reactions occur. By comparing the number of pulses with the number produced by N^{14} (n, p) C^{14} when the chamber contained air, a value of $(1.9 \pm 0.5) \times 10^{-28}$ cm.^{2} for the cross-section for the O^{17} (n, α) C^{14} reaction in ordinary oxygen was deduced. This gives a cross-section of $(0.46 \pm 0.11) \times 10^{-24}$ cm.^{2} per atom of the pure isotope O^{17}. The effect measured is about 20% of the total thermal neutron capture cross-section that has been reported for ordinary oxygen.

Introduction

The thermal neutron capture cross-section of oxygen is known to be very small. Recent experimental determinations have given a value $\sigma = 1 \times 10^{-27}$ cm.^{2}.* Among the possible neutron-induced reactions with any of the three stable oxygen isotopes (O^{16}, O^{17}, O^{18}) which give known products the reaction

$$O^{17} + n^1 = C^{14} + He^4$$

is unique in that it should be exothermic, and so should occur with thermal neutrons. Using the masses given by Mattauch (6), ($O^{17} = 17.00450$, $n^1 = 1.00895$, $C^{14} = 14.00774$, $He^4 = 4.00386$), it may be shown that the above reaction should have a $Q = +1.72$ Mev.

The experiment described in this paper was carried out in an attempt to measure the cross-section for the reaction O^{17} (n, α) C^{14} with thermal neutrons. This will show to what extent it contributes to the total capture cross-section of oxygen, a question that is of special interest because of the use of heavy water as a pile moderator. Since O^{17} is only 0.041% of natural oxygen, the (n, α) reaction would not be expected to compete seriously with simple capture unless the cross-section per atom of the isotope were relatively high, or the capture (n, γ) cross-sections for the more abundant isotopes were extremely low.

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[2] *Member of the United Kingdom Staff until September 1945.*

[3] *Physicist, National Research Council; now at the Chalk River Laboratory.*

* *Report from Manhattan Project, to be published. Table of cross-sections compiled by Dr. H. Goldsmith.*

Experimental Method

The method used was to expose the oxygen (gas) in a pulse ionization chamber to a flux of thermal neutrons, and to look for pulses whose size corresponded roughly to 1.7 Mev. Two oxygen samples of different isotopic constitution were used for most of the experiment; one was enriched (three times normal) and the other impoverished (0.5 times normal) in O^{17}. Differences between counts obtained with the two samples were taken to give a result due to O^{17}. Other devices such as discrimination of pulses according to their energies, variation of the gas pressure in the chamber, and variation of the neutron flux were used to guard against spurious results.

The pulses produced in the chamber were fed through a linear amplifier (gain $\sim 10^5$) to a kicksorter which sorted them according to their size, and counted them on mechanical counters. The kicksorter biases were arranged so that the anticipated energy (~ 1.7 Mev.) fell at about the middle of the range; the probability of distinguishing a small effect due to O^{17} from a large background of both larger and smaller pulses was thus enhanced.

The experiment was carried out twice, the first time in the Montreal Laboratory at the beginning of 1944, with a 'natural' neutron source. Then, in April 1944, the measurements were repeated in the Argonne Laboratory at Chicago, using the much more intense flux of neutrons from the thermal column of the graphite pile. Here much more dependable results with better 'statistics', were obtained and they form the chief basis for the conclusions reached below.

Apparatus

The same apparatus was used for the Montreal and Argonne experiments, except for the kicksorter. (A newly designed instrument was used at the Argonne Laboratory.) The actual experimental runs in the two cases were, however, somewhat different because of the vast difference in the neutron intensities and counting rates.

The ionization chamber was a high-pressure pulse chamber designed by one of the authors (A. N. M.) for fast neutron recoil work. The sectional sketch, Fig. 1, shows the construction. The chamber is cylindrical, with a useful volume of 55 cc. The electrodes—the inner one is the negative ion collector—are of thin copper, and the surfaces facing the collecting volume were silver-plated to reduce the background. Soldering was avoided where possible in the construction, and the soldered joints that remained were very carefully plated to contain any α-particles produced by neutrons on boron or lithium that might have been left by the soldering flux. The polystyrene electrode support was designed to minimize the possibility of the production of pulses in the chamber by recoil protons from fast neutrons.

The negative high tension for the outer electrode was supplied by a 0–5000 volt supply of conventional design with full-wave rectifier and degenerative pentode stabilization. The collecting voltage normally used was between 2000 and 3000 v.

The chamber was evacuated and filled through the filling tube shown in Fig. 1. The method generally used for filling to pressures up to 2 atm. is obvious from Fig. 2. Here C is the chamber, M an ordinary mercury–glass manometer, T a trap that can be immersed in liquid air boiling at reduced

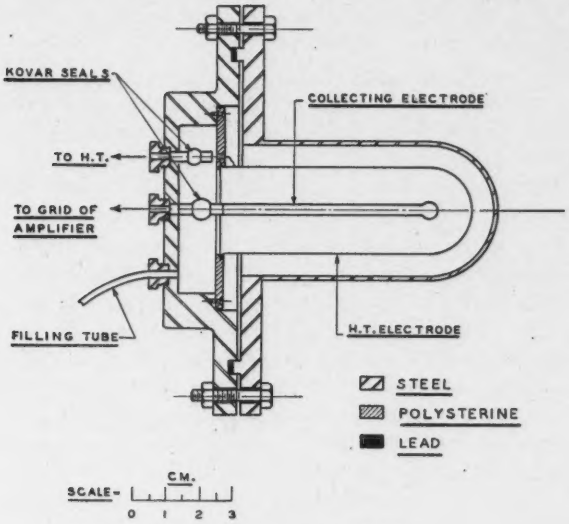

FIG. 1. *Section of pressure chamber.*

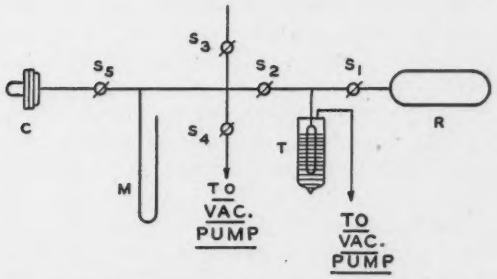

FIG. 2. *Chamber filling system. C, chamber; M, manometer; T, liquid air cooled trap; R, oxygen reservoir;* $S_1 \ldots\ldots S_5$; *stopcocks.*

pressure, R the oxygen reservoir ($1\frac{1}{2}$ litre flask), and $S_1 \ldots\ldots\ldots S_5$ are stopcocks. The following sequence of operations fills the chamber. With the system evacuated to the left of S_2 and between S_1 and S_2 (S_1, S_2, S_3, and S_4 closed), S_1 is opened and oxygen condensed in T. Then S_1 is closed and S_2 opened, and T warmed until the required pressure is reached in the chamber as shown by M. Finally T is cooled with S_2 closed, and surplus oxygen in T

may be boiled off through S_1 back into R. By a similar process the oxygen in the chamber may be almost completely recovered, and returned to R via T. In practice a sequence of counts were taken with alternate flasks of enriched and impoverished oxygen at R, all other conditions remaining constant. Care was taken not to admit any air into the chamber during successive fillings. At the beginning and end of a sequence air could be admitted through S_3 for calibration purposes as described below.

The linear amplifier was one of the standard type used in the Montreal Laboratory. It consisted of five stages with resistance-capacity coupling, and with a variable time-constant 'quick stage'. The first stage is in a separate box placed close to the chamber, and is designed to have low noise and to be non-microphonic. The linear amplifier output could be monitored visually with a cathode-ray oscilloscope.

The pulses from the amplifier were fed to a kicksorter. This instrument sorts pulses into a number of classes according to their amplitude—the limits being determined by pre-set voltages—and records the number counted in each class. The kicksorter used in the Montreal experiments had 12 channels and was built by H. F. Freundlich in Cambridge, England, in 1942. That used at Argonne was a new model (3) built in 1943-44 in the Montreal Laboratory, and was specially adapted to counting the broad pulses produced by the slow collection of ions in oxygen. Twelve channels were used. A separate discriminator and scale-of-32 operated in parallel with the kicksorter served to monitor the total count, and so check against the possibility of losses or double counts.

In the Montreal experiments, since the counting rates obtained with the natural source were of the order of a few per hour, rather elaborate precautions were taken to minimize the number of spurious counts from extraneous electrical and mechanical disturbances. The experiment was carried out in an electrically screened room, and filters were placed in the 110 v. mains supply to cut out pulses that might be fed into the amplifier from outside. Some care was taken to find the best earthing system for the apparatus. Sponge rubber was used liberally to reduce microphonic effects in the chamber and amplifier.

Calibrations of the kicksorter bias voltages in terms of energy were made by introducing air into the chamber, and observing the pulses from the reaction $N^{14}(n, p)C^{14}$. This gave a single point at about 0.6 Mev., which was sufficient, assuming the linearity of the whole system. (The linearity was actually checked by introducing artificial pulses of various sizes on to the collecting electrode and grid of the first tube through a small ($\sim$1 $\mu\mu$f.) condenser.) The number of pulses observed under given conditions could also be used as a measure of the product of neutron flux and effective chamber volume, and permitted an estimation of the cross-section for the oxygen reaction to be made in terms of the nitrogen cross-section.

A schematic diagram of the arrangement of apparatus is given in Fig. 3.

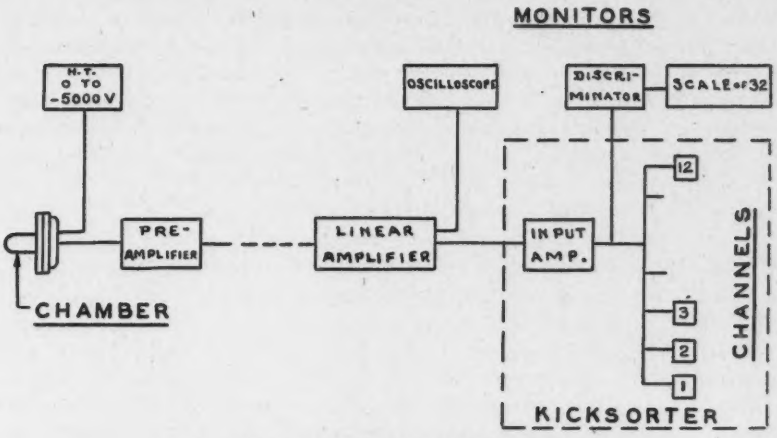

FIG. 3. *Schematic arrangement of counting apparatus.*

Experimental Results

A. *Montreal Laboratory*

A series of measurements was made in Montreal over a period of several months using 2.4 gm. of radium at the centre of an 11 cm. beryllium cube as a source of neutrons. This, together with the chamber, was surrounded by paraffin wax, with some lead between source and chamber to reduce the γ background. Runs were made with both natural (electrolytic) oxygen and enriched oxygen (three times normal in O^{17}), in each case at several different pressures. Each run was repeated with cadmium shielding around the chamber. Measurements were made at night as much as possible to avoid disturbances, and the total length of each run was 25 to 35 hr.

All pulses between about 0.6 and 2.5 Mev. were counted, and cadmium differences taken. Since variations in gain of the amplifier during the long runs were unavoidable, and since the chance of recording spurious counts was high, while the total number of pulses observed was small, the results of this experiment were very uncertain. It did seem apparent, however, that for a given pressure there were more pulses due to C neutrons from the enriched gas than from the ordinary, and that this difference increased with the pressure. The average difference (enriched minus ordinary) was between four and six counts per hour per atmosphere. Since 14,000 counts per hour were obtained from the nitrogen (n, p) reaction under the same conditions with the oxygen replaced by one atmosphere of air, this suggested a cross-section of about 0.0003 times the thermal capture cross-section of nitrogen for a 'twice-enriched' oxygen.

B. *Argonne Laboratory*

In the Argonne experiments the chamber was placed in a hole in the thermal column of the graphite pile, and advantage was taken both of the higher

neutron flux available and of the ease of varying the flux by known amounts. It was decided as a result of the Montreal experiment that the correlation of results obtained at different pressures was unreliable, since the collection of ions in oxygen is slow and it is difficult to ensure that the pulse size for a given energy is independent of pressure. All runs with oxygen were therefore made at the same pressure—2 atm. The gases compared were enriched (three times normal) and impoverished (0.5 times normal) in O^{17}. The nitrogen calibrations were made as before with 1 atm. of air.

Three separate determinations were made of the O^{17} effect. Calling them (I), (II), and (III), they consisted respectively of the following measurements.

(I). With 2 atm. of enriched oxygen, runs were made at pile power levels of 90, 180, 350, and 820 w. The neutron flux at the chamber must increase proportionally. The number of counts per minute in each channel was corrected where necessary for the loss occurring in the mechanical counter. (This correction was made for all the Argonne results, and will be assumed hereafter.) With 2 atm. of impoverished oxygen, similar runs were made at each of the same four power levels, and finally the series was repeated for the enriched gas. When the 'impoverished' values were subtracted from suitably chosen means of the two sets of 'enriched' values (adjusted for variation in amplifier gain), a set of results $N_i(P)$ per min. (enriched minus impoverished) were obtained for each kicksorter channel (number i) at each of four powers. The values $N_i(P)$ for each channel were then plotted against the power P and straight lines were drawn from the origin through the appropriate points. This was generally easy, but an exception occurred in the case of the lowest few channels, where at higher powers the points rose rapidly above a certain power. It was obvious during the experiment that this effect was due to a piling up of small pulses which occurred in great number, even producing an effect in the difference (enriched minus impoverished). Their origin will be discussed below, and the points affected were disregarded in drawing the best straight lines. From the series of straight lines, characteristic values $N_i(1)$ = number of counts per minute at 1 kw. were obtained, and these plotted against the channel number, i, give the spectrum shown in Fig. 4 (A) (Expt. I). Here the majority of pulses occurred at the low end of the kicksorter range, and a considerable part of the curve had to be extrapolated with the assumption that the peak was roughly symmetrical. The area under the peak was then determined to get a measure of the oxygen effect.

Finally, a 'nitrogen calibration' was made with 1 atm. of air in the chamber. The gain of the amplifier was increased to bring the proton pulses into the kicksorter range, and the pile power was reduced to give a reasonable counting rate. The area under the well defined nitrogen peak (Fig. 5) was then corrected to a value corresponding to 1 kw. power and to 2 atm. of pure nitrogen. The ratio of the final result for oxygen to the corrected result for nitrogen gives directly the ratio of the cross-section (σ_E) for a 2.5 times enriched oxygen to that (σ_N) for nitrogen. These results are given in the first line of Table I. From the position of the nitrogen peak, and adopting the value

0.55 Mev. for the disintegration energy of the (n, p) reaction (6), the energy scale shown at the bottom of Fig. 4 (A) was deduced. The oxygen peak falls at approximately 1.4 Mev.

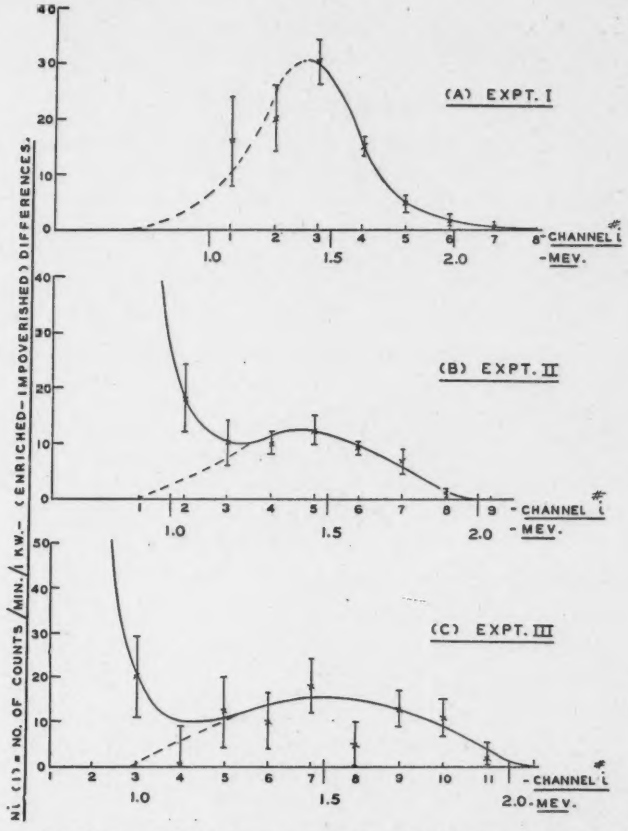

FIG. 4. *Distribution of pulses due to oxygen effect for the three Argonne Laboratory experiments.*

(II). In order to shift the oxygen pulses nearer the middle of the kicksorter range the kicksorter biases were altered, and the experiment repeated. The sequence of operations was exactly as described under (I), except that the second set of runs with enriched gas was omitted. The final spectrum obtained is shown in Fig. 4 (B) (Expt. II). It is apparent that actually little was gained; this time it was quite impossible to allow for the piling up of pulses into the lower channels, and again a large extrapolation had to be made. However, the presence of a peak—again at about 1.4 Mev.—is unquestionable, although a determination of the number of pulses belonging to it is difficult.

(III). Finally, in an attempt to avoid the 'piling up' difficulty, much longer runs were made with both enriched and impoverished oxygen at very low

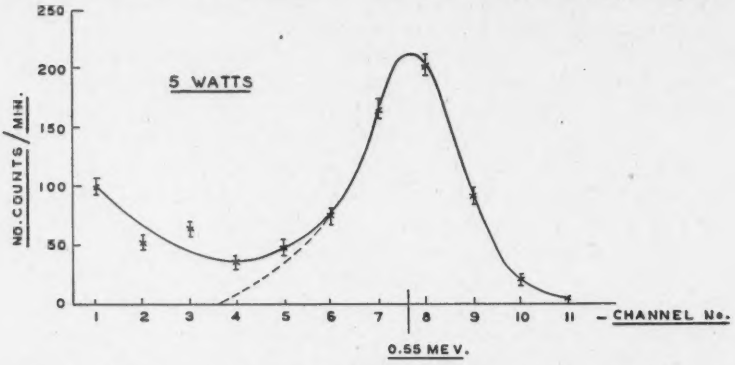

FIG. 5. *Distribution of pulses due to nitrogen (n, p) disintegrations used for calibration (Expt. I).*

power (17.6 w.). The differences are plotted in Fig. 4 (C) (Expt. III), and again a peak is apparent with the low energy rise still present. A run with cadmium surrounding the chamber was also made which verified that the effect observed was due to C neutrons.

The results of Experiments (I), (II), and (III) are summarized in Table I.

TABLE I

Experiment	No. counts/min./1 kw./2 atm.		Ratio $\frac{\sigma_E}{\sigma_N} \times 10^4$	Energy of oxygen peak or Q, Mev.
	Oxygen	Nitrogen		
(I)	94 ± 20	2.94×10^5	3.2 ± 0.7	1.4
(II)	54 ± 12	2.94×10^5	1.8 ± 0.4	1.4
(III)	92 ± 24	2.67×10^5	3.4 ± 0.9	1.5

The mean value of $\frac{\sigma_E}{\sigma_N}$ is $(2.8 \pm 0.7) \times 10^{-4}$, and the mean Q is 1.4 Mev. Taking $\sigma_N = 1.7 \times 10^{-24}$ cm.^{2} per atom,* we get

$$\sigma_E = (4.8 \pm 1.2) \times 10^{-28} \text{ cm.}^2$$

Since this refers to an oxygen enriched 2.5 times in O^{17}, we find, assuming that the whole of the effect measured is due to O^{17}, the cross-section per atom of normal oxygen to be

$$\sigma_O = \frac{4.8}{2.5} \times 10^{-28} \text{ cm.}^2$$
$$= (1.9 \pm 0.5) \times 10^{-28} \text{ cm.}^2$$

* *See footnote, page 77.*

Moreover, from the natural abundance of O^{17} (0.041 %), we deduce the cross-section per atom of the isotope to be

$$\sigma_{O^{17}} = (0.46 \pm 0.11) \times 10^{-24} \text{ cm.}^2$$

It will be noted that this should contribute approximately 20% of the total oxygen capture cross-section that has been reported (1×10^{-27} cm.^{2}*).

Discussion of Results

We have observed an effect produced by thermal neutrons in a pulse ionization chamber containing oxygen. When the concentration of O^{17} and O^{18} is increased, the pulse size distribution shows a significant increase in number between 1 and 2 Mev. with a maximum at 1.4 Mev. This can be attributed to the reaction

$$O^{17}\ (n, \alpha)\ C^{14},$$

which should be exothermic ($Q = +1.72$ Mev.) according to the masses.

The following considerations prompted the assignment of the observed effect to this reaction.

(1). The effect is observed as a difference between results obtained with two samples of oxygen, both produced in the same way by the electrolysis of heavy water. One sample was enriched, and the other impoverished in O^{17}. The only other difference should be in their O^{18} concentration, e.g., the first sample was also enriched (seven times normal) in O^{18}. The effect increases with pressure of the gas in the chamber.

(2). By the use of cadmium shielding, and by varying the neutron flux by known amounts, the effect was found to be due to thermal neutrons.

(3). The effect appears as a peak in the pulse spectrum lying between 1 and 2 Mev., with a maximum at 1.4 Mev. The anticipated energy for the reaction is 1.72 Mev. The discrepancy is not considered to be significant since the following questionable assumptions were made for the experimental energy calibration. The effect of the recombination and slow collection of ions in the chamber on the pulse size was assumed to be the same for 2 atm. of oxygen as for 1 atm. of air; moreover, the number of electron volts required to produce one ion pair was assumed to be the same for the different ionizing particles involved under these different conditions.

The most likely impurity in the oxygen samples is nitrogen, which should produce pulses of only $\frac{1}{2}$ to $\frac{1}{4}$ the size of those observed.

(4). The possibility of other exothermic reactions with the three stable oxygen isotopes has been considered. A summary of all possible (n, p) and (n, α) reactions in oxygen is given in Table II, with their calculated Q's. It will be seen that the reaction O^{17} (n, α) C^{14} is the only one giving a known product that can be exothermic by mass considerations. The O^{16} reactions are excluded anyway by the experimental method.

* *See footnote, page 77.*

TABLE II

Reaction	Mass of oxygen isotope (Mattauch (6))	Mass of product			Q, Mev.
		Mattauch (6)	Empirical	Barkas (1)	
$O^{16}(n, p)N^{16}$	16.00000	16.00645			−5.25
$O^{16}(n, \alpha)C^{13}$	16.00000	13.00756			−2.30
$O^{17}(n, \alpha)C^{14}$	17.00450	14.00774			+1.72
$O^{17}(n, p)N^{17}$?	17.00450		>17.011		<−5
				17.0136	−7.7
$O^{18}(n, p)N^{18}$?	18.00485		>18.011	—	<−5
				18.027	−20
$O^{18}(n, \alpha)C^{15}$?	18.00485		>15.005	—	<+5
				15.0165	−6.1

The last three reactions in the table, if they occurred, would produce unknown isotopes of nitrogen and carbon. It may be readily seen from a table of masses of isotopes that in cases in which the appropriate group of masses is known, the following rule generally holds:

$$M_Z^A - M_{Z+1}^A > M_Z^{A-1} - M_{Z+1}^{A-1},$$

where M_Z^A is the mass of an isotope of element Z with mass number A. (It is true for every known group below $Z = 13$ (Al).) By means of this rule lower limits for the masses of N^{17}, N^{18}, and C^{15} were deduced, and are given in the table as 'empirical' masses. For example,

$$N^{17} - O^{17} > N^{16} - O^{16} = +0.00645$$

and so

$$N^{17} > 0.00645 + O^{17} = 17.011.$$

Barkas (1) has calculated the masses of N^{17} and C^{15} based on theoretical considerations of nuclear binding energies. These are also shown in Table II, along with a value for N^{18} which has been calculated by the authors by the same method. The values of Q corresponding to the various estimated masses are given in the last column. It is apparent that each of the three reactions should be endothermic, with the possible but improbable exception of the reaction O^{18} (n, α) C^{15}.

The value $Q = 0.55$ Mev. for the reaction N^{14} (n, p) C^{14} is quoted by Mattauch (6), and is due to an ionization chamber measurement by Huber, Huber, and Scherrer (4). Livingston and Bethe (5) have calculated the value 0.62 Mev. from the results of Bonner and Brubaker (2), who made range measurements in a cloud chamber. If the higher value (0.62 Mev.) is used for the energy calibration, the oxygen peak is shifted to 1.6 Mev. Such a change is, however, inconsequential, since little accuracy is attached to our energy measurement as pointed out above.

The determination of the cross-section could have been made to a much greater accuracy had it not been for the large background of small kicks that merged with the lower end of the O^{17} peak and was particularly disturbing at

higher powers. That their effect still persisted in the (enriched minus impoverished) differences has already been noted. It seems most likely that this was due to nitrogen impurity in the samples, the contamination being greater for some reason in the enriched gas. Thus there were an appreciable number of pulses belonging to a large nitrogen peak which, owing to background noise, piling up effect, etc., were recorded in the channels up to ~ 1 Mev. or higher. There was also a small background of pulses above 2 Mev. but this remained roughly constant, being eliminated by the (enriched minus impoverished) differences, and may have been due to boron contamination on the chamber walls.

Acknowledgment

We wish to express our gratitude to the Manhattan Project Directors, who made the facilities of the Argonne Laboratory available to us, and to members of the Argonne Staff for their generous co-operation and assistance.

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SONIC DETERMINATION OF THE ELASTIC PROPERTIES OF ICE[1]

By T. D. Northwood[2]

Abstract

By measuring the velocity of various types of elastic waves in a solid it is possible to deduce Young's modulus and Poisson's ratio. Longitudinal, extensional, and Raylelgh wave velocities were measured in ice, the first by resonance in a rod and the other two by a pulsing technique. The value obtained for Young's modulus was 9.8×10^{10} dynes per cm.^{2} and for Poisson's ratio was 0.33.

Introduction

The author was recently concerned with the design of an instrument for measuring the thickness of ice by supersonic echo-ranging. Since it was desirable at the outset to know the velocity of sound in an extended mass of ice, a search was made for velocity data or the elastic constants from which the velocity could be deduced. Some values of Young's modulus were found, and although they were very scattered the more recent ones seemed reliable enough. However, there was no satisfactory information on Poisson's ratio, which was also required in this application. Consequently, it was necessary to make velocity measurements as the equipment developed. In view of the fragmentary nature of the published information it was considered worth while to extend the measurements sufficiently to obtain the elastic constants of ice.

General Method

The expression for the velocity of sound in any medium is

$$v = \sqrt{\frac{\text{Elasticity of medium}}{\text{Density of medium}}},$$

using the modulus of elasticity appropriate to the mode of propagation. In a solid, several modes of propagation, associated with the various elastic moduli, are commonly encountered. For example, the velocity of sound travelling longitudinally in a long wire or thin rod will be given by using Young's modulus in the above formula, whereas in other circumstances the modulus of rigidity or the extensional modulus, or some modification of these, might be applicable.

The elastic moduli may all be expressed in terms of Young's modulus and Poisson's ratio, and it is usual to regard these two quantities as the fundamental elastic constants of a material. Young's modulus may readily be measured directly, but Poisson's ratio is usually determined indirectly, from the relations between the elastic moduli.

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In the present work three types of propagation were studied. The relations between them are discussed briefly below.

The velocity in an infinitely long rod, commonly called the 'longitudinal' velocity, is given by the familiar expression:

$$v_0 = \sqrt{\frac{E}{\rho}}, \tag{1}$$

where E is Young's modulus and ρ is the density of the material.

The velocity through an infinite solid medium ('extensional' velocity) is given by:

$$v_e = \sqrt{\frac{E\,(1-\sigma)}{\rho\,(1+\sigma)\,(1-2\,\sigma)}} \tag{2}$$

$$= v_0 \sqrt{\frac{(1-\sigma)}{(1+\sigma)\,(1-2\,\sigma)}}, \tag{3}$$

where σ is Poisson's ratio.

The transverse wave over the surface of an extended solid is related to the shear wave, which occurs in thin plates. Vibrations of this type were first discussed by Rayleigh (9, p. 441) and are commonly called 'Rayleigh waves'. A mathematical treatment is given by Love (8, p. 307). The velocity may be written:

$$v_r = K \sqrt{\frac{E}{\rho} \cdot \frac{1}{2\,(1+\sigma)}} \tag{4}$$

$$= v_0 \,.\, K \sqrt{\frac{1}{2\,(1+\sigma)}}\,. \tag{5}$$

It differs from the shear velocity by the factor K, a complicated function of Poisson's ratio, which however is readily evaluated for $\sigma = \frac{1}{4}, \frac{1}{3}, \frac{1}{2}$. These three values adequately define the function in the important range of Poisson's ratio. The resulting curve is shown in Fig. 1.

Also shown in Fig. 1 are the ratios v_r/v_0, v_0/v_e, and v_r/v_e. It will be noted that v_r/v_0 is almost independent of Poisson's ratio, and v_r is therefore almost as useful as v_0 for determining Young's modulus. The other two ratios are fairly sensitive to changes in Poisson's ratio, and are therefore used to determine this quantity.

Longitudinal Velocity

The longitudinal velocity was measured by determining the resonance frequencies of long rods cut from the ice. The procedure was simply to drive one end of the rod with a Rochelle salt transducer, and pick up the vibration at the other end with a similar transducer. The driver consisted of an audio oscillator with considerable output power and good frequency stability. The pick-up system consisted of transducer, amplifier, and cathode ray oscillograph. Frequency determinations were made by connecting to the horizontal deflection plates of the oscillograph a beat-frequency oscillator of considerable stability, whose calibration was adjusted frequently against the Research Council's 1000 cycle standard. After the driver oscillator had been tuned to

resonance, the beat-frequency oscillator was adjusted to give a Lissajous figure on the oscillograph.

Equation (1) for longitudinal velocity applies to the case of an infinite rod, where r^2/l^2 is a negligible quantity. It was not feasible to meet this condition,

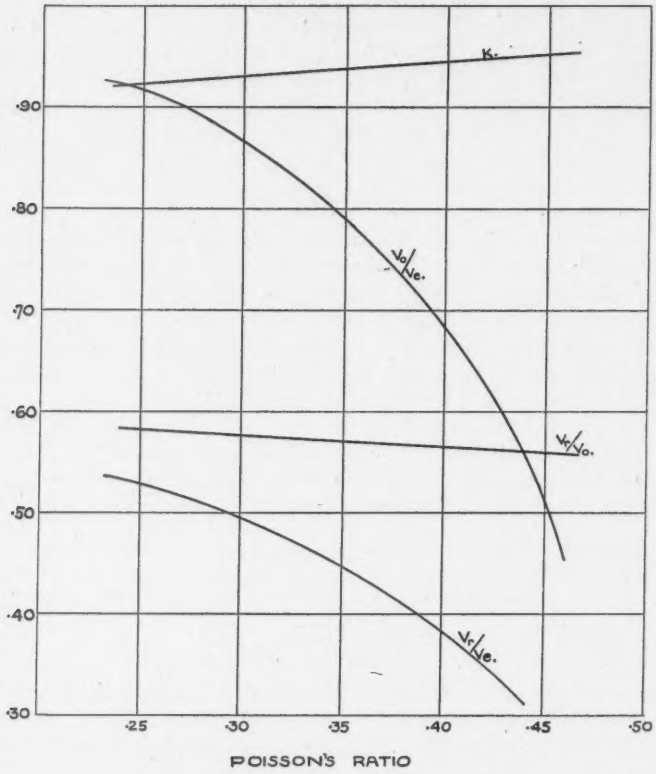

FIG. 1. *Curves showing relations between the three velocities as functions of Poisson's ratio. K is the ratio of Rayleigh wave velocity (v_r) vs. shear velocity.*

and it was therefore necessary to apply the correction worked out by Rayleigh (10, p. 251), who shows that the resonant frequency of a rod is lowered by the factor

$$1 - \frac{\pi^2\sigma^2}{4} \cdot \frac{r^2}{l^2} .$$

The procedure in the present investigation was to start with the longest possible rods and take a series of observations for progressively shorter lengths. These were plotted against r^2/l^2, and the resulting straight line extrapolated to obtain the infinite-rod value, v_0. The maximum length, limited by the

capacity of the available constant temperature chamber, was about 40 cm. While the r/l correction could also be reduced by reducing the diameter of the rod, other complications appeared, such as the end-loading effect of the transducers, which made it advisable to work with a diameter of at least 5 cm.

It was first considered that Rayleigh's correction might itself be used as a method of determining Poisson's ratio, by measuring velocities for a series of values of r/l. However, when r/l becomes large enough to give accurate results, an additional factor, the radial resonance of the rod, begins to affect the velocity (4).

Extensional Velocity

The extensional velocity was measured with the use of a pulsing technique. A 300 kc. pulse was applied by a Rochelle salt transducer on one face of a block of ice and received on the opposite face. The transit time was measured by applying initial and received pulses to an oscillograph. The details are as follows. The pulsing system was an overloaded audio amplifier, converting an audio-frequency sine-wave into a square wave and eventually (via a small coupling capacitor in the last stage) into a series of very sharp pulses. The sharp pulses were introduced on the grid of the overbiased 6AC7 tube whose plate load comprised an r.f. choke in parallel with the crystal transmitter unit. The output circuit was thus shock-excited during each positive pulse on the grid of the 6AC7, producing a damped 300 kc. signal, three or four cycles long, from the transmitter.

The pinging system was driven by a Hewlett Packard Model 205-A oscillator, at as high a frequency as possible without undue 'reverberation' due to multiple reflections within the sample. This was a few hundred cycles in the case of the ice samples.

The ice blocks used were, of course, large enough to provide a practically infinite medium for the 300 kc. pulse, so that Equation (3) applied directly.

Rayleigh Wave Velocity

Measurements of Rayleigh waves were made by the same method as for extensional waves, except that the transducers were both on the same face of the ice block. The separation of the transducers was varied from about 1 to 40 cm., giving a linear relation between pulse-time intervals and transducer spacing, whose slope is the Rayleigh wave velocity.

Miscellaneous Details

The ice samples used for these experiments were cut from the Ottawa River or obtained from a local ice manufacturer. It was assumed that in all cases the crystallization was sufficiently rapid that the ice consisted of many crystals, variously oriented, and could be considered isotropic. This is borne out by the fact that although specimens were cut in a variety of directions from the ice-blocks, no great variation was noted from sample to sample. Such minor variations as were observed could be explained by internal flaws which were present in some specimens.

Temperature was controlled by using a temperature controlled test chamber. The specimens were enclosed in a copper case around which air cooled by passage over 'dry ice' was circulated when necessary. For most of the measurements a temperature of $-15°$ C. was maintained, although some observations of the variation in longitudinal velocity with temperature were made.

Discussion of Results

The following velocity measurements were obtained:

$$v_0 = 3.28 \pm 0.02 \times 10^5 \text{ cm. per sec.}$$
$$v_e = 3.98 \pm 0.02 \times 10^5 \text{ cm. per sec.}$$
$$v_r = 1.85 \pm 0.05 \times 10^5 \text{ cm. per sec.}$$

Poisson's ratio was determined from the ratios v_0/v_e and v_r/v_e using the curves of Fig. 1:

$$v_0/v_e = 0.824, \text{ whence } \sigma = 0.33$$

and

$$v_r/v_e = 0.464, \text{ whence } \sigma = 0.34.$$

Young's modulus was determined from the observed value of v_0:

$$E = 9.9 \times 10^{10} \text{ dynes per cm.}^2,$$

and from the value of v_r and Poisson's ratio:—

$$E = 9.7 \times 10^{10} \text{ dynes per cm.}^2.$$

The extensional and Rayleigh wave velocities, measured by the pulsing technique, were sufficient in themselves to obtain the two elastic constants. However, it was desired to verify the method by obtaining the third velocity, involving as it did a different experimental approach and measurement of the wave velocity rather than the group velocity. The agreement obtained is most gratifying.

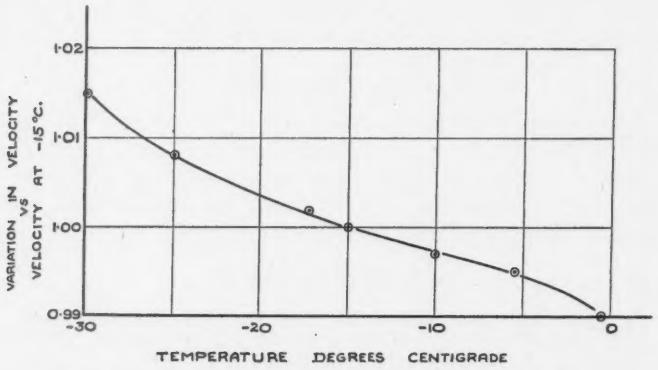

FIG. 2. *Variation in longitudinal velocity with temperature.*

The variation in longitudinal velocity with temperature is shown in Fig. 2. It will be seen that the velocity decreases very slowly with increasing temperature in this range.

Absorption

As mentioned above, the pulsing frequency used in the extensional velocity measurements was limited by reverberation, due to multiple reflections within the ice. In some preliminary work on similar samples of duralumin the reverberation was much worse, and it was therefore concluded that the absorption of the wave in ice was much greater than in duralumin. This suggests the possibility of measuring absorption in solids by applying the ordinary reverberation theory used in architectural acoustics.

It was also observed that the absorption increased greatly as the temperature approached the freezing point.

Reference should be made to the possible effect of dispersion on the pulsing experiments, since group velocities were measured. This subject has been thoroughly dealt with by several investigators (5), and it is known that certain boundary conditions give rise to selective absorption and dispersion. Such conditions were, of course, avoided in the present work. Inspection of the received 300 kc. pulse showed experimentally that no dispersion occurred.

Comparison with Previously Published Results

A wide variety of values for the elasticity of ice have been given by various investigators. Most of the early measurements were made by static methods, and the results were so scattered that there appears to be something inherently wrong with the method (6, 7). Dorsey (2, pp. 444, 398-417) suggests that plastic flow may occur in ice at even the most moderate static loads, thus invalidating the static measurements. However, this does not explain the statical experiments of Koch (7), who, after obtaining two sets of results that did not agree, repeated the work with the same apparatus but with extreme caution about temperature variations. He then obtained a value of Young's modulus that agrees very well with recent sonic measurements.

It is probable that a great deal of the variation could be attributed to the effects of varying temperature. Most of the early work was done only a few degrees below the freezing point, and was dependent upon winter weather for maintaining a steady temperature. In these circumstances the samples were probably subject to a certain amount of thawing from time to time, and would by no means be the perfect single crystals that they were assumed to be. And whereas the sonic method is relatively insensitive to small inhomogeneities, the static method depends greatly on uniformity of the sample throughout.

The most complete sonic measurements of Young's modulus appear to be those of Boyle and Sproule (1), who measured the resonant frequencies of rods, much as was done in the present work. Their results are shown in Table I. They agree substantially with the present work at $-15°$ C., although greater variation with temperature was noted.

The authors remark that they also were handicapped by dependence upon outdoor weather conditions, and that there seemed to be a time lag in the

TABLE I

VALUES OF YOUNG'S MODULUS

Author	Ref.	Temp., ° C.	Orientation of rod with respect to freezing surface	E, dynes/cm.^{2}
Koch	(7)	Not stated	Parallel	9.4×10^{10}
			Perpendicular	11.2
Boyle and Sproule	(1)	−35	Perpendicular	10.9
		−30	"	10.2
		−10	"	9.48
		− 9	"	9.28
Ewing, Crary, and Thorne	(3)	0 to −5	Average of parallel and perpendicular samples	9.17
Author's value	—	−15	Various	9.8

change in E with temperature. This might have been attributed to the slowness of the interior of a sample to arrive at a new ambient temperature, but it was noted that the effect was equally noticeable in large and in small samples. Hence, it would rather appear to be a slow structural change in the ice that occurred after a change of temperature.

In the present work, a similar phenomenon was observed. Results obtained immediately after a temperature change were quite erratic. Hence measurements were made several hours after the temperature had changed.

Measurements were also made by Boyle and Sproule of the variation in elasticity with crystal orientation. However, the variation with orientation appeared to be less than that from sample to sample. Possibly a significant difference could be found if the temperature were held constant.

Work done by Ewing, Crary, and Thorne (3) for temperatures between 0° and −5° C., gives slightly lower results than those of Boyle and Sproule. These are also included in Table I.

From these various results and those reported in the present paper, it would appear that ice is highly susceptible to thermal effects, at least in the range −30° to 0° C. The elasticity seems variable during a period of temperature change, but under steady-state conditions there is no great change with temperature.

In this connection, Dorsey (2, pp. 444, 398-417) has collected a great variety of opinions on the physical structure of ice that illustrate the difficulty of understanding even this relatively simple and very common material. It is considered that the supersonic pulsing scheme used here, combined with a temperature controlled test chamber, would be a useful tool for a detailed investigation of the structural nature of solids, especially near their critical points.

Acknowledgment

The author wishes to acknowledge the advice and criticism of Dr. G. S. Field of this laboratory, whose wide experience with vibrations in solids was called upon frequently during the project.

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THE FOUNDATIONS AND PHILOSOPHICAL IMPLICATIONS OF WAVE MECHANICS[1]

By Seyuan Shu[2]

Abstract

This paper attempts to show:

(*a*) The nature of the basic assumptions made either explicitly or implicitly by L. de Broglie in arriving at the fundamental formula, $\lambda = \frac{h}{p}$, of wave mechanics, and that at least some of these assumptions are open to criticism.

(*b*) That it is a little too hasty to regard as demonstrated the non-localization of photons or electrons in motion and the lack of determinism with respect to them.

(*c*) That the presuppositions which lead to the 'relations of incertitude' involve highly arbitrary elements and these 'relations' are read into or arbitrarily imposed upon the physical world rather than really found to be in it.

(*d*) That it is an unwarranted claim that the new mechanics can contribute to the solution of the age-long philosophical problem of free will.

(*e*) That it is possible to render the foundations of wave mechanics logically independent of the theory of relativity.

I. The Basic Ideas of Wave Mechanics

Since 1927, experimental proofs have been repeatedly obtained by physicists concerning the undulatory nature of electrons (and even of other material particles such as protons and atoms). The scientific world considers therefore as fully established the basic ideas of wave mechanics first propounded by Louis de Broglie during 1923-24. There is no fault to find with these experimental results, nor can one entertain any doubt as to the soundness of de Broglie's main thesis that, like photons, electrons (and indeed other elementary material particles) partake of the nature of waves. But the arguments that de Broglie employs in establishing the formula for the wave-length of a material particle in motion seem to be quite unsatisfactory, and one cannot help deeming highly debatable the foundations of wave mechanics.

De Broglie's formula for the wave-length of a material particle in motion,

$$\lambda = \frac{h}{p},$$

where λ is the wave-length, p the momentum of the particle, and h Planck's constant, can be deduced only by admitting as premises the following four propositions:

(1) Einstein's transformation equation for time-co-ordinates,

$$t' = \frac{t - \frac{vx}{c^2}}{\sqrt{1 - \frac{v^2}{c^2}}},$$

is valid.

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[2] *Correspondent Member of the Chinese National Academy, Peiping. Professor of Philosophy, National Central University, Nanking.*

(2) Einstein has really proved that all material body of mass m_0 has an energy content equal to m_0c^2.

(3) The letter W in Einstein's formula for the photon, $W = fh$, must be interpreted as representing not only the kinetic energy of the photon but also its internal energy. (Einstein deduces from his formula $W = fh$ another formula, $p = \frac{fh}{c}$, p being here the momentum of the photon, by assuming $W = \frac{m_0c^2}{\sqrt{1 - \frac{v^2}{c^2}}}$. But one can also derive the formula $p = \frac{fh}{c}$ from the formula $W = fh$ by regarding W as consisting of nothing but kinetic energy, for the 'internal energy' of a photon does not differ practically from zero.)

(4) The energy (kinetic and internal) of a material particle in motion, which is taken to be equal to $\frac{m_0c^2}{\sqrt{1 - \frac{v^2}{c^2}}}$, must be equal to Planck's constant multiplied by the frequency of its associated wave.

(A)

Here we shall discuss the first point. If Einstein's special theory, or more precisely, Einstein's transformation equation for time-co-ordinates were wrong, it would be impossible for de Broglie to obtain for the phase velocity of the associated wave the formula $V = \frac{c^2}{v}$ from which the formula $\lambda = \frac{h}{p}$ is derived. In fact, de Broglie would then be utterly unable to obtain any formula at all for the so-called phase velocity of a material wave. If it should prove impossible to establish such a formula, there would be lacking at least one of the theoretical foundations for wave mechanics. It has been elsewhere shown (8, Chaps. III and VII) in detail that one is bound to reject Einstein's Special Theory as contrary to concrete reality, and the arguments will not be repeated here.

(B)

With regard to the second point, it must be said that the assertion that a material body of mass m_0 has an energy content of m_0c^2 is an altogether unwarranted extrapolation, far from being a logical consequence of Einstein's Special Theory. Even if one accepts the Special Theory, one can yet refuse to credit such an assertion. For the above assertion to be valid one has to show beforehand that all matter with its elementary constituents, such as neutrons and protons, could be *entirely* transformed into radiant energy with no residue of matter or 'mass at rest'. Clearly, there is no justification for such a statement. It seems likely that a pair of electrons of opposite sign can be transformed into a photon, but the same thing cannot be said of neutrons and protons. In the observed phenomena of material radiation, the protons emitted still preserve their mass m_0, which is not transformed into energy. There is not the slightest empirical basis for the affirmation that the

mass m_0 of a proton or a neutron could be converted into a sum of radiant energy equal to m_0c^2. As the mass of a material object consists almost entirely of its protons or neutrons, evidently it would be idle talk to pretend that all matter could be transformed into energy. (It has been observed that certain stars during their long course of evolution have lost nine-tenths of their original substance. This forms, however, no decisive argument in favour of the possibility of converting protons and neutrons into radiant energy. Stars may have lost part of their substance in protons, neutrons, etc., through material radiation, but there is no indication whatever that these elementary material particles have become either photons or pure mechanical energy.)

In the article first published in 1905, *Does the Inertia of a Body Depend on its Energy-content?* (6, pp. 69-71), Einstein claims to have proved that after an emission of luminous energy equal to L a material body at rest will lose a quantity of mass equal to $\frac{L}{c^2}$. This result is generally regarded as the basis for the assertion that a body of mass m_0 has an energy content equal to m_0c^2. Now, according to the present writer, the 'proof' given by Einstein in the above-mentioned article is altogether unsatisfactory. Einstein has not really succeeded in showing that a quantity of radiant or luminous energy equal to L possesses a mass equal to $\frac{L}{c^2}$. (One could justify the assertion that an amount of luminous energy equal to L possesses a mass equal to $\frac{L}{c^2}$ as being a logical consequence of Lorentz's formula concerning the variation of mass as a function of velocity only by assuming that luminous energy is nothing but a transformation of subatomic *kinetic* energy.) Strictly speaking, Einstein has not been able even to show the inertia of light. Still less is there any ground for the belief that Einstein has proved that matter, or the mass of a body, could be entirely transformed into energy. If there is no theoretical support for such a belief, one has to reject as groundless the assertion that a body of mass m has an energy content equal to m_0c^2.

Even if the 'proof' were rigorous, it could not be applied to the case of material radiation. In this case the protons or neutrons radiated still preserve their 'mass at rest', which is neither transformed into photons nor into pure mechanical energy, while in the case of electromagnetic radiation (including the radiation in the form of visible light) the 'mass at rest' of photons is generally taken to be zero. In the so-called proof, the letter L must represent an energy with no residue of any 'mass at rest'; hence its inapplicability to the case of material radiation, unless it could first be shown that protons or neutrons were capable of being entirely transformed into photons or into pure mechanical energy.

Einstein believes that in his article, *Does the Inertia of a Body Depend on its Energy-content?*, he has proved that a quantity of radiant energy equal to L possesses a mass equal to $\frac{L}{c^2}$. The 'proof' given in this regard is, as we

have said, quite unacceptable. It is far from rigorous, though it seems to be perfectly so. The premises which Einstein uses are:

(1) A certain quantity of radiant energy, if it is L when measured with reference to a 'stationary' system, will be $\frac{L}{\sqrt{1-\frac{v^2}{c^2}}}$ when measured with reference to a system possessing the velocity v (Doppler effect).

(2) A quantity of kinetic energy equal to E possesses a mass equal to $\frac{E}{c^2}$.

The present writer wants to point out that it is illogical to base the proof on the above two premises. In fact, no deduction from these premises could justify even the assertion that radiant energy possesses mass. (It must, however, be remarked that what we reject is the proof given by Einstein; there is no reason to doubt the inertia of radiant energy. It is certain that radiant energy possesses mass, and it is also very likely that a quantity of radiant energy equal to L possesses a mass not differing materially from $\frac{L}{c^2}$).

Let us consider a body emitting a certain quantity of radiant energy. If that quantity is L when the measurements are made with respect to a system in which the emitting body is at rest, it will be $\frac{L}{\sqrt{1-\frac{v^2}{c^2}}}$ when the measurements are made with respect to a system in which the emitting body moves with a velocity v (here one presupposes the validity of the Lorentz transformation). This difference is due to the Doppler effect. We shall now consider it from the point of view of the second system. The body moves with the velocity v, and the sum of radiant energy it has emitted is $\frac{L}{\sqrt{1-\frac{v^2}{c^2}}}$. Since the notion of internal energy is based on the phenomena of radiation and at least a part of radiant energy must be regarded as being but a transformation of internal energy, the internal energy of the emitting body must have naturally decreased after the body has emitted a quantity of radiant energy equal to $\frac{L}{\sqrt{1-\frac{v^2}{c^2}}}$.

But has its kinetic energy also somewhat diminished after the emission? Has one any right to consider the quantity of radiant energy equal to $L\left(\frac{1}{\sqrt{1-\frac{v^2}{c^2}}}-1\right)$ as being entirely or at least in part but a transformation of a portion of the kinetic energy of the emitting body which moves with the velocity v? (The expressions for the same quantity of radiant energy differ according to whether the emitting body is at rest or in motion; the difference between the two cases is $L\left(\frac{1}{\sqrt{1-\frac{v^2}{c^2}}}-1\right)$, which sum may therefore be con-

sidered as being due to the velocity v.) To the two above questions, if one knows the facts, one ought certainly to answer in the affirmative, because in point of fact radiant energy does possess mass. But premises (1) and (2) contain absolutely nothing that might justify one in doing so. From these premises it is impossible for Einstein to draw the conclusion (a) that any emission of radiant energy must have caused a diminution of the kinetic energy of the emitting body (or that a part of the radiant energy $\frac{L}{\sqrt{1-\frac{v^2}{c^2}}}$ must be due to kinetic energy). Einstein has therefore not the slightest justification for making the further conclusion (b) that an amount of radiant energy equal to $L\left(\frac{1}{\sqrt{1-\frac{v^2}{c^2}}}-1\right)$ (which is due to the velocity v) must be regarded as resulting from the transformation of a part of the kinetic energy of the emitting body. ("Due to velocity" is not necessarily the same as "due to kinetic energy". This is a very important distinction.) To affirm (a) would be the same as to affirm the proposition (a') that radiant energy possesses mass. To affirm (b) would be the same as to affirm the proposition (b') that a quantity of radiant energy equal to L possesses a mass equal to $\frac{L}{c^2}$, the very proposition that has to be proved. Propositions (a') and (b') can be deduced from (a) and (b) but not from (1) and (2), the two premises that Einstein has assumed. Premises (1) and (2) are equally consistent with non-(a) and non-(b) as with (a) and (b). In order to arrive at Einstein's conclusions (a') that radiant energy possesses mass and (b') that a quantity of radiant energy L possesses a mass equal to $\frac{L}{c^2}$, propositions (a) and (b) have first to be supposed as true. ((a) and (b) may well correspond to experiment, but they are to be based upon theoretical and empirical grounds other than those that could be deduced from the Doppler effect plus the special theory of relativity.) But in the proof given by Einstein, there is not the least trace of such a presupposition. So one has to reject his proof as entirely unsatisfactory, and even as fallacious.

Now look at the problem from another point of view. In his article (1905) Einstein seems to take it for granted at the outset, perhaps without being fully aware of it, that the 'internal energy' of a body, which is taken to be the cause of radiant energy, must depend on its mass and on its mass alone. Thus, internal energy is considered as a function of one variable, that is, a function of mass, $f(m)$, and as completely independent of the velocity of the body. Now since radiation is a concrete phenomenon, one may admit as legitimate the notion of 'internal energy' (which is to be considered as the cause or source of radiant energy and indeed whose existence is assumed chiefly for the purpose of accounting for the latter). But here one has to deal with an empirical datum, not with a simple question of definition. It would be quite unscientific to determine a priori the factors on which must

depend the so-called internal energy. Of course, one can freely imagine that the internal energy of a body, being the source of radiant energy, may be intimately related to its mass; but until further informed by experimental evidence one has no right to affirm dogmatically that internal energy must be a function of mass and of mass alone. There is not the slightest reason, either theoretical or empirical, which might warrant the assumption that the internal energy must always be the same when measured with regard to different co-ordinate systems. It would be also unjustifiable to affirm that a new sort of energy, if it should depend also on velocity, like ordinary kinetic energy, ought to be called kinetic instead of 'internal'. There is no justification for the omission of the letter v from the formula for a different sort of energy, simply because it has already appeared in the formula for kinetic energy. The notion of internal energy is based on the phenomena of radiation. It serves rather as an explanation for the phenomena of radiation. The idea is that what manifests itself as radiant energy must already exist as a sort of subatomic energy which is therefore termed internal energy. One calls this sort of energy internal, because one supposes it exists within the atom, but this does not suggest that it must be something constant and independent of all reference systems. (The dependence of internal energy on velocity might be physically false, that is, might be contrary to reality; but logically, it is as conceivable as the dependence of mass on velocity. To a superficial observer, it might seem to contradict the principle of the conservation of energy. But here the contradiction is only apparent, not real. 'Internal energy' is only a possible form of energy; it has not actually become energy, so long as it has not been transformed into radiant energy. The principle of the conservation of energy requires that any actual energy lost by a system must produce work; it does not imply that any possible energy lost must also produce work.) It is only in assuming tacitly the dependence of internal energy on mass and on mass alone that Einstein can deduce the result $m_0 = \frac{L}{c^2}$ from the two initial equations: $E_0 = E_1 + L$, $H_0 = H_1 + \frac{L}{\sqrt{1 - \frac{v^2}{c^2}}}$ (see the article to which reference has already been made). If internal energy is a function of both mass and velocity, then one can deduce from the above two equations the following result:

$$m_0 = \frac{L}{c^2}\left(\frac{1}{\sqrt{1 - \frac{v^2}{c^2}}} - \frac{1}{\alpha}\right) \Big/ \left(\frac{1}{\sqrt{1 - \frac{v^2}{c^2}}} - 1\right).$$

(In this formula α is an undetermined function of v. A quantity of internal energy equal to E_0 with reference to the system at rest will be $\frac{E_0}{\alpha}$ with regard to a system possessing the velocity v.)

It is only in the case $\alpha = 1$, that is, only by assuming the independence of internal energy relative to reference systems, that one could, like Einstein,

obtain the formula $m_0 = \frac{L}{c^2}$. In fact, Einstein can by no means even arrive at the conclusion that radiant energy possesses mass, because he has no right to assume α is necessarily different from $\sqrt{1 - \frac{v^2}{c^2}}$. At any rate, nothing justifies Einstein in taking for granted that α must be equal to 1. It is only in assuming $\alpha = 1$ that one could deduce from the above-mentioned two equations the result that "If a body gives off the energy L in the form of radiation, its mass diminishes by $\frac{L}{c^2}$ ". From what has been said above, it will not be difficult to see that if internal energy depends on mass but not on velocity, then propositions (*a*) and (*b*) will be valid. On the other hand, if (*a*) and (*b*) are valid, internal energy must be independent of velocity. In a word, Einstein believes that, if one takes into consideration the Doppler effect, one could deduce as a logical consequence of the special theory of relativity the conclusion that a quantity of radiant energy L possesses a mass equal to $\frac{L}{c^2}$, whereas the contention of the present writer is that the inertia of radiant energy could not be deduced from the special theory, even when one also accepts the Doppler effect.

(C)

We shall now discuss the third point. It is an experimental fact that the formula for a photon or a light-quantum, $W = fh$, agrees well with the measured quantities. But is there any reason to suppose that the letter W in this formula must be interpreted as representing the kinetic energy of the photon plus its internal energy m_0c^2? This interpretation is justifiable only on the condition that one could first establish the proposition that a particle of mass m_0 has an energy content equal to m_0c^2. As has been shown in the preceding section, this proposition is without the least theoretical support. So one is bound to abandon such an interpretation. In fact, in the case of photons, as far as the magnitude of W is concerned, it makes no difference whether one interprets W as representing both the kinetic energy and the 'internal energy' m_0c^2, or as representing simply the kinetic energy, because the mass at rest of a photon is generally considered as being infinitely small and its 'internal energy', the term m_0c^2, does not differ practically from zero. It is therefore evident that if the formula $W = fh$ as interpreted by Einstein agrees well with experiment it must also agree with experiment when one interprets it as representing only the kinetic energy of the photon. But one has to reject Einstein's interpretation, because it involves at least the entirely unwarranted affirmation that a particle of mass m_0 has an energy content equal to m_0c^2.

We must here add a remark. If one interprets W as consisting simply of kinetic energy, one can also deduce from the formula $W = fh$ the other

formula, $p = \frac{fh}{c}$, which is generally regarded as being confirmed experimentally by the Compton effect. As the formula $p = \frac{fh}{c}$ can be obtained without reference to the notion of internal energy, it is plain that the Compton effect can never be considered as constituting a proof for the assertion that a body or a particle of mass m_0 has an energy content equal to m_0c^2.

(D)

According to de Broglie, the formula for the energy of a material particle will be the same as Einstein's formula for photons: $W = fh$. Here de Broglie is guided simply by analogy. As pointed out above, in the case of photons W can be interpreted as consisting only of kinetic energy without causing any conflict with experiment. But with regard to de Broglie's formula of the same form, if one is bound to interpret W as consisting only of the kinetic energy of the material particle, the result would be disastrous for de Broglie's theory (because the wave frequency of the material particle would thus be zero whether the particle be at rest or in motion). De Broglie found it necessary to interpret the letter W (in his own formula as well as in Einstein's formula for photons) as representing not only the kinetic energy but also the internal energy m_0c^2 of the particle. But de Broglie has no right to make such an interpretation, unless the statement that a particle of mass m_0 has an energy content equal to m_0c^2 could be justified. Now this statement, as has been shown in Section (B), is quite groundless. So one has to reject de Broglie's formula $W = fh$, W being interpreted by its author as representing the total energy $\frac{m_0c^2}{\sqrt{1 - \frac{v^2}{c^2}}}$ of the material particle.

It has been pointed out at the beginning of this essay that the fundamental formula of wave mechanics, $\lambda = \frac{h}{p}$, can be deduced only by admitting as premises the four propositions, which have been dealt with one by one but none of which appears to be valid. Now if the formula $\lambda = \frac{h}{p}$ is without real theoretical foundation, the whole edifice of wave mechanics with its whole set of Schroedinger's equations, as well as the formulae expressing the so-called relations of incertitude such as $\Delta x \, \Delta p \geq h$, would be nothing but conjecture.

It cannot be denied that de Broglie's formula $\lambda = \frac{h}{p}$ does possess some empirical value. What the present writer has tried to do is only to show the emptiness of the reasons given by de Broglie for arriving at that formula. It is therefore de Broglie's theory only that is here repudiated.

It was doubtless a rare intuition to have surmised the undulatory nature of electrons. As in the case of photons, the formula $\lambda = \frac{h}{p}$ agrees well with

experiment—it has been remarked that this formula can also be obtained without assuming the 'internal energy' m_0c^2 of a particle of mass m_0—so de Broglie might on the simple strength of analogy assume this formula to be valid also in the case of electrons. In an address delivered in Stockholm, de Broglie said that with regard to the formula for the total energy of an electron it is "natural" to write it as $W = fh$. It would apparently be no less "natural" to write the formula for the wave-length of an electron, directly, as $\lambda = \frac{h}{p}$. This would certainly much simplify the matter.

II. Determinism or Indeterminism

According to the present writer, rigorous determinism is an abstract ideal like a geometrical entity such as a 'straight line', or a 'circle', etc. Man's experiments are by their very nature incapable of yielding any proof that rigorous determinism does really exist in nature, just as no experiments can prove the perfect realizability of certain geometrical entities. The possibility of rigorous determinism depends at least on the condition that with respect to every particle in motion one could always speak of it as having a definite position at every particular instant and describing a definite trajectory. Now the utmost one can say is that, so far as ordinary physical phenomena are concerned, their course of evolution seems to observe a rigorous determinism.

The recent developments in microphysics tend to break up the rigid frames of the time-honoured scientific determinism. In fact, the only empirical basis that seems to justify such a radical change of opinion is a series of experiments that seem to indicate that photons (or electrons), even when arriving *one by one* at the interference apparatus, can still produce the phenomena of interference (2, pp. 152-164; 3, pp. 31-36, 61-63). If in these experiments the phenomena of interference are really due to photons (or electrons) which come singly one after another in contact with the apparatus, and if it could also be shown that these photons (or electrons) must have identical initial states—if these two conditions are fulfilled, one is bound to recognize that photons or electrons in motion are not only non-localizable but also essentially 'non-localized', and one has to admit frankly that no rigorous determinism exists, at least with respect to photons or electrons in motion. But if only the first of the above two conditions is fulfilled, that is to say, in the case that the initial states of the photons or electrons in question may not be identical, then one may still cherish the hope of being able to determine, from the particular initial states (supposed to be knowable) of a photon or an electron, the particular spot on the posterior screen of the interference apparatus where the photon or electron must finally show itself. In this case a photon or electron in motion could not be considered as non-localizable or non-localized, except when it passes through the two tiny holes on the anterior screen. Still there will always be a mystery, something contrary to expectation, if photons or electrons can arrive singly at the interference apparatus and yet produce the phenomena of interference. For though it must be the tiny

holes that let the photon or the electron pass, yet one cannot maintain that it passes through either one of these two holes. One is forced to admit that when the photon or the electron comes near the two holes it behaves as if it could pass through the two holes at the same time, thus relinquishing, at least for the moment, its particulate nature and becoming 'non-localized'.

In short, if it is empirically certain that photons or electrons possessing identical initial states and arriving singly at the apparatus can produce the phenomena of interference, then the non-localization of photons or electrons in motion must be regarded as proved; and, as the classical scientific determinism is incompatible with the non-localization of particles, one has to abandon rigorous determinism at least with respect to photons or electrons in motion. But who could guarantee the absolute singleness of photons or electrons arriving successively at the apparatus in the experiments and also the identity of their initial states? No doubt, it would be a little too hasty to regard as demonstrated the non-localization of photons or electrons in motion and the lack of determinism with respect to these particles.

III. The Relations of Incertitude

The so-called relations of incertitude depend at least on the validity of the following propositions:

(*A*) What is associated with a particle in motion is not a single monochromatic wave but a group or a packet of superposed monochromatic waves.

(*B*) The variations of position and velocity (or momentum) of a particle can be adequately represented by the expansion or contraction of its associated packet of superposed monochromatic waves.

With regard to propostion (*A*), one must readily acknowledge that "nothing compels us to suppose that the wave Ψ (the wave associated with a particle) must be a monochromatic wave; it may well consist of . . . . . the superposition of plane monochromatic waves and forms a train of waves" (4, pp. 198-199). But the reason here given by de Broglie is purely negative. One might say with equal reason: "Nothing compels us to suppose that what is associated with a particle in motion must be a group of superposed monochromatic waves; it may well consist of a single monochromatic wave". One is here free to choose between the alternatives; but, evidently, the supposition that only a single monochromatic wave is associated with the particle is the simpler of the two. De Broglie could give no better reason than the one cited. Another reason has indeed been given but it is not more convincing. It is urged that a group of superposed monochromatic waves is necessarily limited in space, while a single wave can theoretically fill the entire space; so, according to de Broglie, it would seem better to associate with a moving particle a group of waves, instead of a single wave. This argument is certainly not cogent. A wave of infinite extension is but an abstract ideal; every concrete single wave is always limited in space. Every concrete wave has always a

beginning, and within a definite time the wave must have a definite space-limit. There is not the slightest ground to suppose that when one wants to associate a single monochromatic wave with a concrete moving particle, one would have to associate with it an abstract wave of infinite extension, instead of a concrete monochromatic wave of varying dimensions.

With regard to proposition (*A*), the conclusion is as follows:

So far as theoretical considerations are concerned, with a moving particle one may associate a group of waves or a single wave, as one pleases. In this case there is not the slightest reason for preferring a group of waves to a single wave. It is completely arbitrary to associate with the particle a group of waves instead of a single wave, although this arbitrariness does not constitute a valid argument against the procedure.

With regard to proposition (*B*), a little reflection shows that it depends on proposition (*A*) as one of its necessary premises. In fact, proposition (*B*) asserts that a particle in motion, such as a photon or an electron, must have a multiplicity of probable positions and of probable velocities, that is, the particle is not only non-localized but also has no definite velocity. We have analysed the necessary conditions of the notion of the non-localization of photons or electrons. Now, though it would be difficult to regard a non-localized particle as having a definite velocity, yet there is no basis whatever (either theoretical or empirical) for maintaining that a non-localized moving particle must have at every instant a multiplicity of probable velocities. This is not a logical consequence but an arbitrary interpretation of the proposition (*A*) that the associated wave must be a group or a packet of superposed monochromatic waves. Doubtless, it is only because quantum physicists would endow the particle at every instant with a multiplicity of velocities that they have devised the very ingenious and curious method of associating with it a packet of superposed monochromatic waves. But here a sort of arbitrary interpretation is needed for the purpose.

In short, nothing suggests that one must associate with a moving particle a packet of superposed monochromatic waves, and it is certainly no less arbitrary to interpret the artificially introduced packet of monochromatic waves as signifying that the particle in question has a multiplicity of velocities or rather a multiple velocity.

Proposition (*B*) asserts the multiplicity of position as well as the multiplicity of velocity with regard to a particle in motion and also the particular relations between these two multiplicities. Its validity involves necessarily an antagonism of precision concerning the measurements relative to the position and the velocity of an electron in motion. If one accepts as true proposition (*B*) and also de Broglie's formula $\lambda = \frac{h}{p}$, then one can logically deduce from them the well known formulae expressing the so-called relations of incertitude, such as $\Delta x \, \Delta p \geq h$, etc. Briefly, these formulae imply at least the validity of the following assertions:

(1) Photons or electrons, even when such particles arrive one by one at the interference apparatus, can produce the phenomena of interference.

(2) The initial states of the successively arriving photons or electrons mentioned in the preceding assertion are identical.

(3) One must associate with a moving particle a packet of superposed monochromatic waves, instead of a single monochromatic wave.

(4) The fact of its being associated with a packet of superposed monochromatic waves means that the particle has no definite velocity but a multiplicity of probable velocities (or rather a multiple velocity).

(5) De Broglie's formula, $\lambda = \frac{h}{p}$, is well founded theoretically.

Now as regards assertion (1), some element of doubt may still be possible because of the extreme smallness of photons and electrons. Can one truly be sure that a certain visible effect is due to the action of a *single* photon or electron without making any hypothesis? Assertion (2) is a pure assumption. Assertions (3) and (4) are also purely arbitrary assumptions. As to assertion (5), though one cannot deny that the formula has a certain empirical value, yet it has been deduced by de Broglie from premises that are certainly fallacious (see Part I of the present essay). So one must infer that the so-called relations of incertitude and the well known formulae expressing them are invalid. There is not the slightest ground for believing them to have been well established either theoretically or empirically. It would be an egregious error to maintain that these 'relations' and the formulae expressing them are the necessary consequences of the existence of the quantum of action. In order to make the matter clearer, it is advisable to add the following remarks.

The existence of the 'relations of incertitude' was first pointed out by Heisenberg and Bohr, and they have given besides some examples of measurements, each tending , according to them, to show the reality of these supposed 'relations.' Most of the examples given by de Broglie in the 12th chapter of his *Introduction à l'Etude de la Mécanique Ondulatoire* (2, pp. 152-164) have been advanced by Heisenberg and Bohr. Now, according to the present writer, it is impossible to arrive at the relations of incertitude without first making the assumption, among others, that the associated wave is not a single monochromatic wave but a group of superposed monochromatic waves. Among the examples given by de Broglie, some are logically correct only because in these examples one has made either explicitly or implicitly the above assumption. Where no such assumption is made, the argument must necessarily prove to be false. In the 12th chapter of de Broglie's book the examples given in Sections III and IV all depend on that assumption. In the second section (pp. 167-168) one finds reference to a modified form of the well known theoretical microscope originally conceived by Heisenberg. In this form there is no trace of the above-mentioned assumption, but the argument is certainly false. It states: $mv_x\, mv'_x = \alpha \frac{h}{\lambda}$. Now α is a magnitude that can be determined, if λ and v are known (in fact, α can be as small as one likes),

and it cannot be said to have necessarily an incertitude of 2ϵ. If the value of α is known with greater accuracy, obviously here the formula $\Delta x \Delta p \geq h$ cannot be deduced.

In the first section of the 12th chapter, de Broglie argues in general terms that in the determination of the position of a particle by means of a microscope one must necessarily arrive at the relations of incertitude. We try to show the fallacy involved in his argument.

There are three different conditions:

(1) The modification of position that an electron undergoes as the result of the impact of a photon.

(2) The uncertainty in regard to the position of an electron when illuminated and viewed through a microscope.

Condition (1) must not be confounded with condition (2). They are completely independent.

(3) The modification of momentum of the electron due to the impact of a photon.

If one takes Δx to be the modification of position, then (1), *certainly not* (2), is to be represented by Δx. Condition (2) could be represented by Δx, as in the formula $\Delta x \Delta p \geq h$, only on the condition that (2) is to be regarded as the limits of a packet of superposed monochromatic waves associated with the electron. If one does not here assume such a packet of waves, one has to admit that Δp, the modification of momentum of an electron, is in proportion to Δx, the modification of its position. Thus the precisions concerning Δx and Δp would be convergent instead of antagonistic. (In the present example, de Broglie talks of (1), the modification of position, while in fact what he has in mind is (2).)

Conclusion

The advent of the new mechanics has rendered, according to its initiators, all forms of rigorous determinism untenable. Ever since, many learned people talk passionately about 'indeterminism' (or indeterminacy), but have no very precise ideas. In fact, one can attach at least three different meanings to the same word 'indeterminism' with regard to the new mechanics:

(1) According to de Broglie (4, p. 125) an experiment, carried out by Taylor, shows that interferential phenomena can be produced by photons that come singly one after another into the apparatus. If the usual interpretation of this experiment is correct, then a single particular photon (or electron), when it encounters no material body, or at least when it comes into the neighbourhood of the two tiny holes pierced on the fore-screen of the interference apparatus, will cease to behave as a localized particle and will manifest only its undulatory aspect. (For, if the photon or electron in question is then still a particle, it ought to traverse one and only one of the two holes, but experiments seem to prove that the photon or electron may have come to the other side of the screen through the two holes at once, yet without

having passed *through* either of these holes.) According to the accepted interpretation of the Taylor experiment (or any similar experiment with electrons), one has to recognize that photons (or electrons) are, sometimes at least, not only non-localizable but also non-localized. Now rigorous determinism requires first of all that one can speak of a particle or a material body as occupying a definite position at any given instant. According to J. J. Trillat (9, pp. 23-32) the undulatory nature is not peculiar to photons and electrons; even protons and neutral atoms behave like waves. If one supposes that the protons and atoms imprisoned in a material body do not differ essentially from those concerned in radiation, one would then have to admit that not only elementary particles but also physical bodies are not always strictly localized, which admission might be considered as a fatal blow to rigorous determinism. Thus one might speak with some apparent reason of indeterminism, when one deems as being proved by facts the non-localization of photons and material particles.

(2) But, when one speaks of indeterminism, what one has in mind may be not only the non-localization of particles; one might also take it for granted that the photons or electrons which come one by one into the interference apparatus must all be identical with respect to their inner structures, initial states, etc. Consequently one has to accept not only indeterminism but also essential probabilism, or rather to consider this sort of probabilism as forming a part of indeterminism. Thus understood, the word 'indeterminism' will have a richer content than has been envisaged in the preceding case.

(3) Still, one might prefer to base the doctrine of indeterminism on Heisenberg's relations of incertitude. Understood in this sense, the term 'indeterminism' will appear to be yet richer in content, for then it must involve as its premises not only the non-localization of particles and essential probabilism, but also other assumptions. We readily acknowledge that rigorous determinism is incapable of any empirical proof; but the arguments for indeterminism are at least equally far from convincing. To be able to arrive at the relations of incertitude, one has to make a large number of postulates, none of which is entirely justifiable. One can maintain a sort of essential probabilism only on the condition that one presupposes the absolute identity of photons (and also of electrons), which is evidently an altogether unwarrantable assumption. In fact, the mechanism of radiation yet remains unknown. Photons (and also electrons) individually may be very dissimilar; the structures of these particles may be no less complex than those of living organisms. At the present stage of scientific knowledge it would certainly be rash to affirm that photons (or electrons) passing through an interference apparatus can be identified with respect to their size, structure, initial state, etc. So nothing compels one to accept as scientifically established that sort of essential probabilism concerning the eventual localizations of photons or electrons in motion.

In what concerns the phenomenon of radioactivity, which one among the myriads of atoms will next explode is regarded by some physicists of the

newest school as a problem of pure probability. According to these scientists, though the average life of a particular atom can be calculated fairly accurately, yet there is absolutely no reason why one atom among the myriads should explode earlier than another. In their opinion, the myriads of atoms, of which some explode at this very instant and others will explode only thousands of years hence, are absolutely identical with respect to both inner and outer conditions. The present writer cannot but think this supposed identity to be pure conjecture. It would certainly be more reasonable and more discreet to recognize that there must be an adequate reason why a particular atom explodes just at this instant and at no other. The fact that people do not at present know anything about such a reason does not mean that it is non-existent. At least there is no reason to believe that all atoms, electrons, and photons should belong to the same pattern and be absolutely identical. To accept as true a sort of inherent and fundamental probabilism with respect to the localization of photons or electrons, or with respect to the explosion of particular atoms, would be the same as to declare that so far as certain micro-physical phenomena are concerned no determining factors can ever be assigned, that is, the principle of sufficient reason must be here in default (which would mean that an event might happen without an adequate cause). Plainly, it would be more prudent to avoid such an extremist attitude, and it would be more in the interests of science to avow one's ignorance as to the causes by which certain phenomena are produced than declare such causes to be non-existent.

As to the non-localization of photons (or electrons), all depends on the interpretation of the Taylor experiment. Since photons (even if such particles do exist as individuals) are inconceivably small, could one be sure that certain interferential phenomena are really produced by photons which come singly one after another into the apparatus? At least some doubt is here permissible. One might put also the question whether the elementary particles (such as electrons, protons, etc.), when regarded as the constituents of a material body, are the same as those in the free state. From the fact that there are no photons as individuals within the atom, one might argue by analogy that there are no electrons as individuals within the atoms. One might perhaps conceive the photons, electrons, etc. (which are capable of interferential phenomena), as a sort of physical entity separate and distinct from matter, and in any case as not being the elementary constituents of material bodies. If one could thus argue, the indeterminism of particles in the free state would be inapplicable to material bodies and their constituents. In fact, one might reasonably consider photons, electrons, etc., as belonging to a particular class of physical entities that behave under certain conditions like waves and under others like particles; they are at times *exclusively* waves and at others *exclusively* particles. If they are sometimes not localized, it is simply because they are then not particles—there is nothing surprising in this. When they are waves, they must be considered as being nothing but waves. Clearly, the simple truism that waves are waves, not particles, must not be turned

into an argument for indeterminism. (If one considers a photon in propagation to be exclusively a sort of wave, it may well come to the other side of the screen of an interference apparatus without having had to choose one of the two tiny holes in the screen for its passage through.)

Our conclusion is: Though no experiment can ever establish rigorous determinism, just as no experiment can prove the perfect realizability of geometrical entities such as straight lines or planes (because principles or ideas created by the mind aim at absolute precision, the very condition unobtainable in experimental work), yet the arguments the new physicists can offer for indeterminism contain invariably some arbitrary or even artificial elements, which render these arguments far from convincing.

APPENDIX I

Comments and Controversies

(A) Sir A. S. Eddington

According to Sir A. S. Eddington, determinism has been definitely disproved by the so-called relations of incertitude. The foregoing essay has pointed out the arbitrary and artificial elements which are involved in the premises of these relations. Yet Eddington's chief grievance against determinism is not that it has been found incompatible with the new mechanics. He thinks the belief in determinism to be utterly unfounded and gratuitous and holds the opinion that the physicist is not acquainted with a single law in the physical world that might justify the belief in determinism (5, p. 11).

Now, though determinism (as opposed to essential probabilism) or the principle of causality cannot be rigorously proved by experiment (Eddington is not exacting in this regard), yet one is bound to accept it as the fundamental principle of scientific knowledge. No science is possible without admitting as true the principle of causality in one form or another. So far as wave mechanics is concerned, indeterminism or probabilism (inherent and fundamental) may mean that, in the microscopic world, the principle of causality (which may be regarded as affirming that identical causes must produce identical effects) is no longer valid. In the foregoing pages the writer has dwelt on the fragility of the reasons given in support of the essential probabilism and the so-called relations of incertitude. If one adheres to this sort of probabilism the following question naturally presents itself: Is this probabilism rigorous or not? There are three possible answers: (1) It is absolutely rigorous, (2) sufficiently rigorous, or (3) not rigorous at all. An example will be given to make this clear.

We shall, first of all, elucidate somewhat the meaning of probability in connection with that sort of essential probabilism envisaged by the new microphysics. In what concerns the play of heads or tails, for instance, the use of the theory of probability does not mean that there is no certainty or no strict determinism; it is simply an extremely convenient and also the only possible method of calculation, in view of the complexity and minuteness of

the causes. This differs completely from the inherent and essential probabilism of the new microphysics. According to the latter, for a particle, such as a photon or electron, to appear finally at a particular point is a question of pure probability; even should one know all the possible data concerning the physical world at that instant, one would still be unable to predict with certainty the behaviour of a single photon or electron; here the probabilism must be regarded as inherent in the nature of things. If the probability that an individual photon (or electron) will appear at a certain point is calculated to be $\frac{1}{n}$, it is not at all certain that among n identical photons one will eventually arrive at that point; but when an extremely great number, say a billion, are concerned, one can be pretty certain that almost exactly the fraction $\frac{1}{n}$ of this number of photons will show themselves at that point. The movement of an individual photon is entirely unpredictable; one cannot explain why it appears precisely there and not elsewhere. But in all macroscopic phenomena, the submicroscopic indeterminism is completely masked and everything happens as if there exists rigorous determinism. In pre-quantum physics, even where microscopic phenomena alone are concerned, identical causes must produce identical effects and identical effects must be attributed to identical causes: whereas, according to the quantum school, in the domain of microphysics identical causes may produce very dissimilar effects (for photons or electrons which are assumed to be identical in all respects, when emitted by the same source, will show themselves at different points on the rear screen of the interference apparatus)—so here one is bound to accept a certain kind of indeterminism; but, so far as macroscopic phenomena are concerned, identical causes will produce extremely similar, though not absolutely identical, effects (for, when one is concerned with an extremely great number of photons or electrons, the laws governing the probabilities of distribution are always approximately, if not rigorously, verified)—this explains why for practical purposes all physical phenomena may be considered to be completely determined. One might therefore say that, according to the new mechanics, microscopic phenomena are indetermined, while ordinary physical phenomena are approximately determined. But since macroscopic phenomena are made up of microscopic phenomena, one might regard the latter as having some subtle connection with the laws governing the former. Perhaps even the microphysical world is not indetermined to such an extent as the new physicists suppose. At all events, it is only partially true that the new mechanics is incompatible with determinism. And one has also to recognize that if one accepts the new mechanics the principle of causality seems to be invalidated only so far as microscopic phenomena are concerned, and this principle must be deemed to be at least approximately true with regard to ordinary physical phenomena. (Besides, if one accepts only a sort of probabilism with respect to the localization of photons or electrons and rejects the relations of incertitude, it will be at least logically possible to conceive the laws governing microscopic physical phenomena as rigorously deterministic.)

According to de Broglie, the interferential phenomena in the Taylor experiment are really produced by photons which come singly one after another across the forescreen of the interference apparatus. And according to Eddington (5, p. 12) "by means of an instrument called Geiger Counter, electrons can in fact be counted one by one in the same manner as an observer counts, one by one, the stars in the firmament." If their opinions are reliable in this respect, one has doubtless to regard photons or electrons as endowed with individuality. But, on the other hand, if one gives full credit to Pauli's principle, one must deny individuality to electrons. Perhaps, it would be wise to consider this problem as still unsolved. Langevin would save rigorous determinism with respect to all physical phenomena by denying the individuality of elementary particles, but unfortunately he could give no decisive arguments against results obtained with the Geiger counter or in the Taylor experiment.

(B) Planck, Langevin, Einstein

Planck does not think rigorous determinism or the principle of strict causality could be proved, but considers this principle to be the safest guide in scientific research (7, p. 259). Planck is less vehemently anti-indeterministic than Einstein, who regards the very concept of indeterminism as illogical. The latter's opinion is clearly untenable. Rigorous determinism is nothing but one of man's mental constructions. It can neither be proved by experiment nor be deduced by pure logical reasoning. Since determinism has no logical necessity, its opposite, indeterminism, involves at least no logical contradiction. Since determinism is neither empirically nor logically proved, one is bound to admit indeterminism as an idea whose realization is possible at least within certain limits.

People like Langevin and Einstein apparently want to save rigorous determinism at all costs, but the arguments given are sentimental rather than logical. The new physicists, while denying strict determinism to microscopic phenomena, emphasize, however, the fact that, practically, one may regard all ordinary physical phenomena as being subject to a sufficiently rigorous determinism. It would, consequently, be incorrect to consider even the most ardent advocates of the new mechanics, such as Bohr and Heisenberg, to be thoroughgoing indeterminists. Even these two physicists cannot dispense with a sort of approximate determinism. It is not the indeterminism with respect to microscopic phenomena, but unlimited and thoroughgoing indeterminism, which would be incompatible with science. (The latter type would deny completely the principle of causality, and, if it should prove to be true, no prediction with regard to natural phenomena would be possible and cosmos would in no way differ from chaos.) The chasm between Langevin and Einstein on one hand, and Bohr and Heisenberg on the other, would be much less wide than the chasm between the latter two and a thoroughgoing indeterminist, to whom scientific laws would be impossible. In short, whether the concrete physical world is subject to strict determinism or not remains in our opinion an open question; yet one may well be assured that wherever there

are mathematical formulae, there must be presumed to exist some sort of determinism.

Among the initiators of indeterminism the most prominent are Bohr, de Broglie, and Heisenberg. It has been shown in the course of this essay that most of the arguments for indeterminism given by the above-mentioned physicists are far from satisfactory. It is now time to point out a very misleading and unfounded affirmation originally made by Bohr and Heisenberg and often thoughtlessly repeated by physicists of the quantum school. They maintain that the principle of indeterminacy and, in particular, the so-called relations of incertitude have arisen from the fact that physical phenomena are inevitably altered by observation, that is, no pure objectivity is possible; they thus attribute a highly metaphysical significance to their doctrine of indeterminism. Nothing supports such an interpretation. The relations of incertitude are entirely contained in the two postulates, namely, the association of a packet of superposed monochromatic waves with a moving particle and the interpretation of the modifications of its position and velocity by the contraction or expansion of the packet. In all this there is absolutely no connection with the age-long metaphysical problem of the objectivity of our knowledge concerning the external world.

APPENDIX II

Indeterminism and Free Will

The problem of free will is one of the central problems, perhaps the most difficult, of Western philosophy, for it was Kant who said that to solve this problem "centuries have laboured in vain". Since Heisenberg first made known in 1927 his so-called principle of indeterminacy (or relations of incertitude, as de Broglie prefers to call it) not only many idealist philosophers have exulted at the supposed death-blow dealt at the adversary of free will (they have even gone so far as to think indeterminism scientifically proved), but also some prominent scientists have come to hold firmly the opinion that the possibility of moral freedom is for the first time demonstrated by the mathematical formulae of the new mechanics. Sir A. S. Eddington was inclined to think that the year 1927 would mark an important epoch in the development of scientific thought, for, in his opinion, since 1927 religion has become acceptable to a reasonable, scientific mind. One sees how a close relation is supposed to exist between the problem of free will and the principle of indeterminacy of the new physics. Now, according to the writer, this principle, even if it is fully established, does not in the least prove the reality of free will and has in fact nothing to contribute to the solution of that problem.

To refute free will as an objective fact (not the mere consciousness of freedom), one has to make the two following postulates: (1) The world (including living beings) is governed by physico-chemical laws, and (2) all physico-chemical laws are strictly deterministic. If there are non-physico-chemical laws (for example, 'laws of configuration or structure', which might

perhaps be needed to account for the divers behaviours of living beings, if one adopts something like a holistic or emergent-evolutionist point of view), then postulate (1) is denied and one has to admit the possibility of free will, even if postulate (2) is still valid. In case postulate (1) is false and (2) is true, then there will also be possibility but no certainty of free will, for moral freedom might manifest itself as a sort of 'higher' law which would of course conflict with physico-chemical laws but in no way interfere with the supposed rigour of the latter, just as electric force might move a cable car to the summit of Mont Blanc without interfering with the rigour of the law of gravitation. In this case free will is possible, but here it will have nothing to do with the indeterminism of Heisenberg. If postulate (1) is true and (2) is false, then freedom is possible but not certain; it might have its cause in the lack of rigour of physico-chemical laws. Now, if one admits as valid Heisenberg's principle of indeterminacy or the relations of incertitude as well as postulate (1), one has yet no right to regard free will as automatically established. For, according to the new mechanics, indeterminism is manifested only in individual submicroscopic phenomena; with respect to macroscopic physical phenomena there exists a sufficiently rigorous determinism. Now indeterminism limited to the domain of microphysics does not permit freedom of will, unless here one supposes the existence in nature of a non–physico-chemical principle which could reinforce and enlarge the microscopic indeterminism. But such a principle is denied by postulate (1). It is evident that so long as one assumes postulate (1) to be valid, Heisenberg's relations of incertitude are unable to establish the possibility of free will. If one rejects postulate (1), then one ought to admit the possibility of free will, even if Heisenberg's doctrine is refuted. One has therefore to repudiate as groundless the affirmation that the alleged indeterminism with respect to individual microphysical phenomena is to be regarded as a scientific argument for moral freedom.

If the principle of causality should be completely denied, then no phenomena could be explained or predicted, and there would exist no connection whatever between physical events (one would thus have to accept universal freedom or chaos, not merely the freedom of will, for in the complete absence of causality any phenomenon could succeed any other phenomenon). Yet this would be, evidently, also incompatible with the relations of incertitude.

APPENDIX III

Can Wave Mechanics be Rendered Independent of the Theory of Relativity?

The equations (of propagation) of wave mechanics may assume two forms, one non-relativistic, proposed by Schroedinger, and the other relativistic, advanced by Dirac. But whatever form they assume, its basic ideas must have, according to de Broglie, an intimate connection with Einstein's theory. In the first section of the present essay a complete analysis has been given with regard to the presuppositions that de Broglie has made in order to

arrive at his equation for the energy of an electron in motion: $\lambda = \frac{h}{p}$ (the equation that serves as the foundation of wave mechanics). De Broglie accepts in this regard: (i) the Lorentz transformation equation for time-co-ordinates, (ii) the assertion that a body of mass m_0 has an energy content equal to m_0c^2, and (iii) Einstein's interpretation of the letter W in the equation (for a photon or light-quantum) $W = fh$, while (iv) he asserts that by reason of analogy it is very natural to write the equation for the energy of an electron in motion also as $W = fh$.

It has been shown in Section I that there is no reason to accept the first three points, and that one might regard the letter W in Einstein's equation as representing only kinetic energy. As to the fourth point, inasmuch as de Broglie thinks it very natural by reason of analogy to write the equation for the energy of an electron in exactly the same form as Einstein's first equation for photons; one may by the same reasoning write the equation for the wave-length of an electron directly as $\lambda = \frac{h}{p}$ (which is Einstein's second equation for the photon derived by him from the first). This procedure would certainly be no less natural. Thus one might arrive at the fundamental equation of wave mechanics without making the three above-mentioned premises. In case these three premises should prove unjustifiable, it would still be possible to preserve that fundamental equation; and one might speculate on the possibility of rendering the bases of wave mechanics wholly independent of relativity.

Doubtless, all relativists would declare unanimously the impossibility of detaching the bases of wave mechanics from the theory of relativity. It would doubtless be urged that Einstein's second equation (concerning the wave-length of a photon), $\lambda = \frac{h}{p}$, could be deduced from the first equation $W = fh$ (no matter how one interprets the letter W) only by assuming the validity of the Lorentz transformation. According to the writer, this opinion, though apparently well founded, is contestable. To deduce the equation $\lambda = \frac{h}{p}$ from the equation $W = fh$, Einstein has to make use of the equation

$$W = \frac{m_0c^2}{\sqrt{1 - \frac{v^2}{c^2}}} \quad \left[\text{or } E = m_0c^2 \left(\frac{1}{\sqrt{1 - \frac{v^2}{c^2}}} - 1 \right) \right],$$

and it is indispensable for him to suppose the dependence of mass on velocity which is a consequence of the Lorentz transformation. There is no doubt that the increase of mass in proportion to velocity is a law that has been amply verified by experiment, and one might regard it as a fundamental physical fact. But this dependence of mass on velocity is no theory but a fact discovered several years before the special theory of relativity was first announced. One has certainly the right to contemplate it apart from relativity and, as it is a fact, also apart from all scientific theories (including Lorentz's theory). It

has been shown (8, Chap. III) that one must reject the special theory of relativity, and it has also been made clear (8, Chap. I) that if Einstein's theory is incorrect, Lorentz's must automatically be rejected. But the increase of mass in proportion to velocity is, let it be repeated, no theory but a fact that cannot be denied. It might well be a fundamental fact, irreducible and irrational. Anyhow, there is absolutely no need to 'explain' it by a false theory or to regard it as one of its 'verifications'. When some physical facts seem to be more or less fundamental, one must either accept them such as they are (because really fundamental facts do not admit of further explanations), or avow frankly one's ignorance for the moment with regard to their possible explanations. Now, if mass should be deemed to be something constant as in classical physics, then from the first equation for photons, $W = fh$, one would derive the second equation as $\lambda = \frac{1}{2}\frac{h}{p}$, which is certainly incorrect, since the variability of mass has been empirically proved. But one may write the second equation for photons as $\lambda = k\frac{h}{p}$ (k is here an unknown constant), and by reason of analogy one may assume this equation to be valid for electrons also. Now k must be greater than $\frac{1}{2}$, at least this is certain. As to its exact magnitude, experience alone can determine it. To all appearances, it is approximately 1 (as C. Guye's experiments seem indirectly to show), but one is unable to say that it must be 1, unless one accepts either Lorentz's or Einstein's theory. (It must be remarked that from Lorentz's or Einstein's theory one can deduce the formula $k = 1$, but not the converse). But one will certainly be justified in assuming $k = 1$, at least provisionally, as a conclusion directly based on facts. Thus one can render the bases of wave mechanics wholly independent of relativity. (Einstein's theory is decidedly unsatisfactory, but the variation of mass as a function of velocity is a fact. When one deals with particles having an extremely great velocity, doubtless one has to adopt something like the relativistic equations.)

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